

habits of the hyæna, the great preponderance of his remains in the cave earth, and their absence in the breccia, it seems impossible to avoid the conclusion that he was not an occupant of Britain during the earlier period.

The acceptance of this conclusion, however, necessitates the belief (1) that man was resident in Britain long before the hyæna was.

(2) That it was possible for the hyæna to reach Britain between the deposition of the breccia and the deposition of the cave earth. In other words, that Britain was a part of the Continent during this interval.

Sir C. Lyell, it will be remembered, recognised the following geographical changes within the British area between the Newer Pliocene and historical times (see "Antiquity of Man," edition 1873, pp. 331, 332).

Firstly, a pre-Glacial Continental period, towards the close of which the Forest of Cromer flourished, and the climate was somewhat milder than at present.

Secondly, a period of submergence, when the land north of the Thames and Bristol Channel, and that of Ireland, was reduced to an archipelago. This was a part of the Glacial age, and ice-bergs floated in our waters.

Thirdly, a second Continental period, when there were glaciers in the higher mountains of Scotland and Wales.

Fourthly, the breaking up of the land through submergence, and a gradual change of temperature, resulting in the present geographical and climatal conditions.

It is obvious that if, as I venture to think, the Kent's Cavern breccia was deposited during the first Continental period, the list of mammalian remains found in it should not clash with the list of such remains from the Forest of Cromer, which, as we have just seen, flourished at that time. I called attention to these lists in 1874, pointing out that according to Prof. Boyd Dawkins ("Cave-Hunting," p. 418) the forest bed had at that time yielded twenty-six species of mammals, sixteen of them being extinct, and ten recent; that both the breccia and the forest bed had yielded remains of the cave-bear, but that in neither of them had any relic or trace of hyæna been found. A monograph on the "Vertebrata of the Forest Bed Series" was published in 1882 by Mr. E. T. Newton, F.G.S., who, including many additional species found somewhat recently, but eliminating all those about which there was any uncertainty, said: "We still have forty-nine species left, of which thirty are still living, and nineteen are extinct" (p. 135). Though the number of the species has thus been almost doubled, and the presence of the cave-bear remains undoubted, it continues to be the fact that no trace of the hyæna has been found in the forest bed, and no suspicion exists as to his probable presence amongst the eliminated uncertain species.

It should be added that no relic or indication of hyæna was met with in the "Fourth Bed" of Brixham Windmill Hill Cavern, believed to be the equivalent of the Kent's Hole breccia.

I am not unmindful of the fact that my evidence is negative only, and that raising a structure on it may be building on a sandy foundation. Nevertheless, it appears to me, as it did ten years ago, strong enough to bear the following inferences:—

1. That the hyæna did not reach Britain until its last Continental period.

2. That the men who made the Palæolithic nodule-tools found in the oldest known deposit in Kent's Cavern arrived during the previous great submergence, or, what is more probable—indeed, what alone seems possible unless they were navigators—during the first Continental period. In short, I have little or no doubt that the earliest Devonians we have sighted were either of Glacial, or, more probably, of pre-Glacial age.

It cannot be necessary to add that while the discovery of remains of hyæna in the forest bed of Cromer, or any other contemporary deposit, would be utterly fatal to my argument, it would leave intact all other evidence in support of the doctrine of British Glacial or pre-Glacial man.

Some of my friends accepted the foregoing inferences in 1873, while others, whose judgment I value, declined them. Since that date no adverse fact or thought has presented itself to me; but through the researches and discoveries of others in comparatively distant parts of our island, and especially in East Anglia, the belief in British pre-Glacial man appears to have risen above the stage of ridicule, and to have a decided prospect of general scientific acceptance at no distant time.

I must, before closing, devote a few words to a class of workers who are "more plague than profit."

The exuberant enthusiasm of some would-be pioneers in the question of human antiquity results occasionally in supposed "discoveries" having an amusing side; and not unfrequently some of the pioneers, though utter strangers, are so good as to send me descriptions of their "finds," and of their views respecting them. The following case may be taken as a sample:—In 1881, a gentleman, of whom I had never heard, wrote, stating that he was one of those who felt deeply interested in the antiquity of man, and that he had read all the books he could command on the subject. He was aware that it had been said by one palæontologist to be "unreasonable to suppose that man had lived during the Eocene and Miocene periods," but he had an indistinct recollection that another eminent man had somewhere said that "man had probably existed in England during a tropical Carboniferous flora and fauna." He then went on to say, "I have got that which I cannot but look upon as a fossil human skull. I have endeavoured to examine it from every conceivable standpoint, and it seems to stand the test. The angles seem perfect, the contour the same but smaller in size than the average human head; but that, in my opinion, is only what should be expected if we assume that man lived during the Carboniferous period, in spite of what Herodotus says about the body of Orestes." Finally, he requested to be allowed to send me the specimen. On its arrival it proved, of course, to be merely a stone; and nothing but a strong "unscientific use of the imagination" could lead any one to believe that it had ever been a skull, human or infrahuman.

It may be added that a few years ago a gentleman brought me what he called, and believed to be, "three human skulls and as many elephants' teeth," found from time to time, during his researches in a limestone quarry. They proved to be nothing more than six oddly shaped lumps of Devonian limestone.

So far as Britain is concerned, cave-hunting is a science of Devonshire birth. The limestone caverns of Oreston, near Plymouth, were examined with some care in the interests of palæontology as early as 1816, and subsequently as they were successively discovered. The two most famous caverns of the same county—one on the northern, the other on the southern shore of Torbay—have been anthropological as well as palæontological studies; and, as we have seen, have had the lion's share in enlarging our estimate of human antiquity. The researches have, no doubt, absorbed a great amount of time and of labour, and demanded the exercise of much care and patience; but they have been replete with interest of a high order, which would be greatly enhanced if I could feel sure that your time has not been wasted nor your patience exhausted in listening to this address respecting them.

#### JOSEPH-ANTOINE-FERDINAND PLATEAU.

THE career of this indefatigable investigator, as we announced last week, has just closed. Born in the second year of the present century, he has occupied a notable position in the scientific world for more than fifty years. Before he reached middle age he met with the terrible misfortune of losing his eyesight while trying venturesome experiments on the physiological effects of light. His scientific career seems to have become only more active in consequence! When we think of the ease and success with which certain chess-players can, blind-fold, carry on some dozen or two simultaneous games, there seems little to surprise us in the mathematical career of Euler after he became blind. But the difficulties which stood in the way of the physicist, and which he successfully overcame, were of a far more formidable character. Had his chief investigations related to sound, the loss of eyesight might have but little interfered with them. But to carry out by the help of others' eyes a long series of investigations connected with Physiological Optics was a triumphal feat with which we know nothing to compare, except, perhaps, the composition of those marvellous master-works which Beethoven elaborated after he had become stone-deaf.

Plateau's really great contributions to physical science were, however, not optical, but molecular. They were collectively republished in 1873 in two volumes, with the title, *Statique expérimentale et théorique des Liquides soumis aux seules Forces moléculaires*. This work was

appreciatively reviewed in our columns (vol. x., p. 119) by the then greatest authority on the subject, the late Prof. Clerk Maxwell, so that it is unnecessary for us to analyse it here. Few of the readers of the recently published biography of Maxwell can have forgotten the humorous but accurately expressive lines in which he alludes to this work:—

“And just as that living Plato, whom foreigners nickname Plateau,  
Drops oil in his whiskey and water—for foreigners sweeten it so:—  
Each drop keeps apart from the other, inclosed in a flexible skin,  
Till touched by the gentle emotion evolved by the prick of a pin,” &c.

When we look at the Royal Society's Catalogue, we find that up to 1873 Plateau is credited with fifty-three papers on subjects of the most varied character. One large section of these, of course, forms the matter of the volumes already mentioned. Another large section is devoted to the persistence of visual impressions, subjective impressions of colour, irradiation, and other questions of physiological optics. In connection with these, there are several controversies and reclamations, with and against authorities such as Chevreul and von Helmholtz. In these contests, it must be confessed that Plateau usually has the worse. In fact, he appears very much in the same light as did Brewster a little earlier. He furnished to others, who knew how to interpret and to use them, a great array of novel facts: but his strength lay mainly in the patience and ingenuity which led him to these facts; not in the power of interpreting, explaining, or generalising them.

Besides the two main subjects above mentioned, we find in Plateau's *répertoire* a number of curiosities taken from widely different branches of science. Thus we have a chemical analysis of the mineral waters of Spa; the geometrical problem of describing an equilateral triangle whose several corners shall be on three given circles in one plane; arithmetical recreations; photometry; the “ghosts” produced by various series of rotating spokes; and a centrifugal air-pump.

Plateau occupied with success, until practically disabled, the Chair of Physics in the University of Ghent; and, if he did not attain to the foremost rank among experimental physicists, he at least did much good and useful work under circumstances which would have effectually closed the career of many men who have been more successful than he. He was occupied in his later years in compiling a valuable catalogue of all the papers he could meet with which bore on his special optical inquiries. It is to be hoped that the as yet unpublished part of this collection has been left in a state approaching completion.

#### OFFICIAL REPORTS ON CHOLERA IN EGYPT

**S**URGEON-GENERAL HUNTER, who was commissioned by the Government to make inquiry as to the circumstances attending the cholera epidemic in Egypt, has sent two reports to the Foreign Office. Neither pretends to afford full information on the subject which has been under investigation, but the more recent one, which gives information up to August 19, supplies some indication as to the opinion Dr. Hunter has formed with regard to the etiology of the epidemic. In his first report Dr. Hunter gives the cholera deaths registered up to July 31 as 12,600, but he adds that, owing to defective registration, the total mortality will probably be found to have been nearly double that number. Since that date some 15,000 more deaths have been registered, and if the same faulty system of registration has been maintained, the total mortality up to the present date cannot have fallen far short of some 55,000. The inquiry undertaken by Dr. Hunter relates therefore to a matter of the greatest magnitude, the more so as Egypt has apparently been free from cholera ever since 1865. It is however precisely this question of immunity from cholera that will be raised by Dr. Hunter, and already we are able to gather what opinion will be expressed on this point.

Thus, the possibility of the importation of the disease into Egypt from India is discussed, and it is stated that even some of those who originally were firmly convinced of this method of origin have been forced to a different conclusion. The spontaneous origin of the contagium is also regarded as not being supported by facts; and Dr. Sierra, in a communication which is appended to Dr. Hunter's, distinctly asserts that such a generation of the infection in the Nile Delta cannot be regarded as proved merely because the choleraic germ is often produced at the mouth of the Ganges. Prominence is, however, given to the fact that Egypt has been visited by five epidemics since that of 1831, namely, in 1848, 1850, 1855, 1865, and 1883, and independent testimony is brought forward to show that during the early part of the present year, as also at occasional intervals since 1865, there have been cases of a disease known as “cholérine,” which have been characterised by some of the symptoms of true cholera. And further, Dr. Hunter, in expressing an opinion as to these cases, says that he has arrived at the conclusion that many of them were “what in India we should call cholera.”

A further step in the argument is embodied in a description of the filthy conditions under which the Egyptians live, and especially of the foul state of the Nile at Damietta and other places, both owing to the floating carcasses of animals who had died of bovine typhus and otherwise. Having regard to all these points, the report implies that a number of cases, which for the moment we may describe as sporadic cholera, have formed a somewhat continuous series of attacks ever since the 1865 outbreak, and that the potency of the infection for spread in an epidemic form was developed under the influence of the foul conditions which obtained immediately antecedent to the date of the last epidemic. This view is by no means a new one; it was specially dealt with in a series of papers which were brought before the Epidemiological Society in 1878, when the possibility of a “progressive development of the property of infectiveness” under favourable conditions was insisted on; and it is more than probable that, as regards some of the infectious diseases, it may turn out to be a true explanation of their origin.

It must, however, be borne in mind that in England, and indeed in all thickly peopled countries, cases which are clinically of a similar character constantly occur during the warmer months of the year; indeed, the term “English cholera” is of by no means infrequent occurrence in our mortality tables. And not only so, but Dr. Sierra, in arguing against the spontaneous development of the contagium under the conditions which were found at Damietta, says that the same “cosmo-telluric conditions” have appeared often enough at the mouth of the Nile, that the same accumulation of carcasses in the river has before now taken place, and yet that no cholera has broken out in Egypt. The evidence mainly needed with a view to support the theory which is foreshadowed in Dr. Hunter's reports, should go to point out what were the peculiar conditions which, during the past summer, led to the development of a special potency for mischief in a disease which is always more or less present. The subject is one of the greatest scientific interest, and we trust it will be fully dealt with in the final report.

#### NORDENSKJÖLD'S GREENLAND EXPEDITION

**B**ARON NORDENSKJÖLD telegraphed as follows to the *Times* from Thurso on Friday night:—“An inland ice party started on July 4 from Auleitsvik Fjord. When they were 140 kilometres east of the glacier border and 5000 feet above the sea level they were prevented by soft snow from proceeding with sledges. They sent the Laplanders further on snowshoes. These advanced 230